be admitted as conceivable that on general grounds a man may prefer to estimate the figure of merit of a correlation as measured by r^2 and not by r; but this does not give him the right to say that if the terms of one group of figures are r times those of another, the ratio of one group to the other is r^2.

4. The error creeps in when Krichewsky replaces z_1 by $\beta_{01}z_0$ or rz_0; for when forecasting it is z_1 that is given and the estimated value of z_0 is z_0+e, the error being independent of z_1. But the mean value of z_1 would be rz_0 if we were forecasting z_0 from z_1 by an equation $z_1=rz_0+f$, and the error f in that forecast would be independent of z_0, which is quite a different matter. If it were legitimate to replace a quantity by its mean value under different conditions we could apparently carry the process further and derive the impossible equation $z_0=rz_1=r^2z_0=r^3z=r^4z_0= \cdot \cdot \cdot$

But if we replace z_1 by rz_0+f, to which it is equal, and note that the standard deviation of f is $(1-r^2)^{1/2}$, we see that the standard deviation of $r\ (rz_0+f)$ is r times the standard deviation of $(r^2z^2_0+f^2)^{1/2}$, or $r\ \ (r^2+1-r^2)^{1/2}$, which is r not r^2.

A further point is that the proof of the r law just given holds whether or not there are other factors not independent of z_1.

5. The only argument with which I am acquainted for wishing to estimate relationships by r^2 rather than r is that if a quantity were controlled by two independent factors the total relationship would then be got by adding the component relationships. To this the reply is that in meteorology independence is the exception not the rule. If pairs of forces acting on a particle were always at right angles it might in the same way be urged that the effect of a force should be estimated by its square in order that the resultant might be estimated by the sum of the forces. Now, in estimating the value of a method of forecasting the proportion to which the forecast is controlled by the known data is in my opinion the vital feature, and I should not regard it as more justifiable to adopt r^2 rather than r because it would have points of convenience in exceptional cases than I should to measure forces by the squares of their present measures for a similar exceptional convenience.

NOTE ON THE THEOREMS OF DINES AND WALKER

By EDGAR W. WOOLARD

Let x_0, x_1, be the departures of any two varying quantities; and let the (unknown) complete and exact functional relation in which they are involved be

$$F(x_0, x_1, x_2, \cdot \cdot \cdot \cdot \cdot \cdot \cdot)=0, \tag{1}$$

in which F may be of any form, and in which the x_i may be mutually dependent in any manner, or in part mutually independent.

From a number of pairs of corresponding observed values, we may always compute σ_0, σ_1, and r. Furthermore, for any individual pair we can always write

$$\frac{x_0}{\sigma_0}=r\frac{x_1}{\sigma_1}+b, \tag{2}$$

because a value can always be assigned to b so that this equality will be satisfied; similarly we can always write

$$\frac{x_1}{\sigma_1}=r\frac{x_0}{\sigma_0}+b'. \tag{3}$$

Also, for any *given fixed* value of x_1, we can always find B such that

$$\frac{\bar{x}_0}{\sigma_0}=r\frac{(x_1)}{\sigma_1}+B, \tag{4}$$

and for any *given fixed* x_0 we can find B' such that

$$\frac{\bar{x}_1}{\sigma_1}=r\frac{(x_0)}{\sigma_0}+B', \tag{5}$$

in which $\bar{x}_0$, $\bar{x}_1$, are the *means* of the values of one variable associated with a *fixed* value of the other. The curves

$$x_0=r\,\frac{\sigma_0}{\sigma_1}\,x_1,\ x_1=r\,\frac{\sigma_1}{\sigma_0}\,x_0, \tag{6}$$

are the straight lines of "best fit" (in the sense of least squares) to the individual observations and to the means. However, the fit may or may not be close, and in either case there may or may not exist systematic departures from it; b, B, may or may not be independent of x_1, e. g., and certainly will not if x_1 is not independent of $x_2, \cdot \cdot \cdot \cdot \cdot \cdot$ The standard deviations of b, b', are each $(1-r^2)^{\frac{1}{2}}$.

The preceding equations do not, by themselves, permit any conclusions whatever to be drawn concerning relations of cause and effect; they apply to mere covariation only.

Sir Gilbert Walker has defined the correlation coefficient r as "the proportionate extent to which the variations of each of two quantities are determined by, or related to, those of the other," whence "if there is a cause A and a result M with a correlation r between them, then in the long run A is responsible for a fraction r of the variations of M." The exact meaning intended to be conveyed by this statement is to be found in the mathematical reasoning by which the theorem is supported:

If, in (2), b *is* independent of x_1, then the part of the variation of x_0 which is controlled by x_1 is $r\,\frac{\sigma_0}{\sigma_1}\,x_1$, and the standard deviation ("square mean") of this controlled part is r times the standard deviation, or mean variation of x_0. From this it appears that Walker adopts the standard deviation as a measure of variation and intends his theorem to state that a fraction $r\sigma_0$ of σ_0 is due to variations in x_1, and the remainder $(1-r)\sigma_0$ to variations in $x_2, \cdot \cdot \cdot \cdot \cdot \cdot$ Clearly, this implies not only that x_1 is independent of the remaining variables, but also that x_0 and x_1 are linearly related, so that b is a function only of $x_2, \cdot \cdot \cdot \cdot$; in this case, the first term on the right of the identity

$$\sigma_0{}^2=r^2\sigma_0{}^2+(1-r^2)\sigma_0{}^2 \tag{7}$$

is, by (2), the fraction of $\sigma_0{}^2$ due to x_1.

Now, Dines's theorem states that "if there is a cause A and a result M with a correlation r between them, then in the long run A is responsible for r^2 of the variation in M." Again, the exact meaning intended must be sought in the mathematical proof offered for the theorem:

Substitute (3) in (2):

$$x_0=r\sigma_0\left[r\,\frac{x_0}{\sigma_0}+b'\right]+b\sigma_0. \tag{8}$$

If b is a function only of $x_2, \ldots\ldots$, the first term on the right is the contribution from x_1; for any given *fixed* x_0, the *average* of this term is given by (5), and we have

$$(x_0) = r\sigma_0\left[r\frac{(x_0)}{\sigma_0} + B'\right] + b\sigma_0, \tag{9}$$

in which the first term is the contribution due *in the long run* to x_1. *If*, as frequently happens, B' is practically zero, then

$$(x_0) = r^2(x_0) + b\sigma_0, \tag{10}$$

in which the first term on the right is the *average contribution*, from x_1, to the *particular* value (x_0). Apparently, Dines's theorem is, or should be, intended as a statement of (10).

Clearly, the S. D. of (x_0) is not σ_0, which would seem to dispose of Walker's objection to Dines's theorem. Krichewsky has pointed out, moreover, that Dines's theorem may be interpreted to be a statement of equation (7), in which case it becomes identical with Walker's theorem when allowance is made for the fact that Walker adopts the S. D. as a measure of variation, while in (7) the variation is measured by the square of the S. D., or variance. Implicit in the theorem as thus interpreted, however, is the assumption of the independence of x_1 and the other variables; such independence is the exception rather than the rule. By equation (2) we can always divide the variance in the manner shown in (7); and we may regard the theorem of Dines and Walker as always holding for mere *covariation*. Unless x_1 is independent, however, the law does not hold for *cause and effect*. Walker does not seem to recognize the important distinction between these two cases. Krichewsky has attempted to provide a measure of *causal* influence, even when $x_1, x_2, \ldots\ldots$ are mutually dependent.

NOTES AND ABSTRACTS

INFLUENCE OF PRECIPITATION CYCLES OF FORESTRY [1]

The author made an analysis of the annual radial growth rings of trees in northern Idaho. White pine was the species studied. The trees were located in the Priest River watershed of the Kaniksu National Forest and the stumps of recently cut trees were used. In order to have a wide dispersion of age, five age classes were investigated, viz, 280, 230, 180, 140, and 75 year old trees were measured in each of the five groups, 8 to 15 dominent trees being measured in each group.[2]

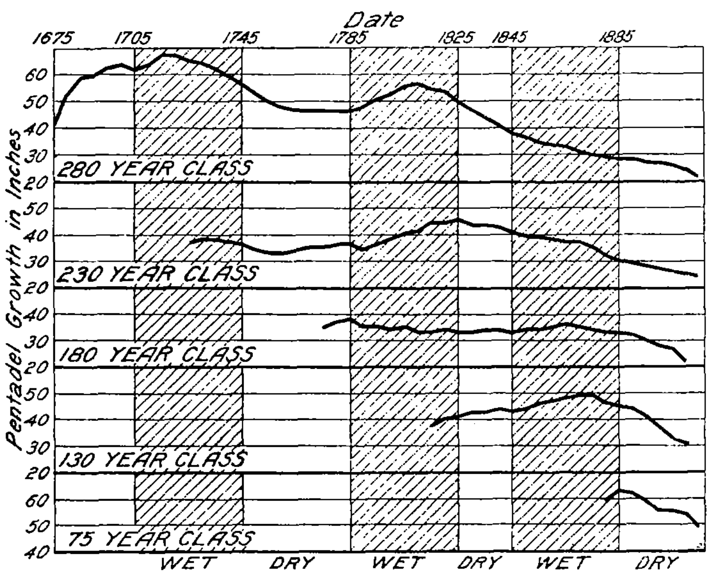

FIG. 1.—White pine growth, 1675–1925

The author plots the 5-year growth for each age class and smoothes the graph so formed by the use of a five-pentad moving mean. By this method general trends are made to stand out much clearer than in the unsmoothed means.

[1] Read by Robert Marshall before the Northern Rocky Mountain Section, Society of American Foresters and printed in Journal of Forestry, Vol. XXV, No. 4, April, 1927.

[2] The author's method of tree-ring measurement as described in a personal letter to the editor was as follows: I first examined each stump and chose some radius which was fairly close to the average in length, showed no growth abnormalities, such as are occasioned by knots, fire scars, insects, or other causes. * * * Along the radius I placed a narrow strip of stout paper and made a small pencil mark along the edge where each ring came. I worked, of course, from the outside to the center of the tree, so that I might date each year's growth. Western white pine is not characterized by double rings, so that difficulty in stem analysis was obviated. In the office I measured the distance between the marks on my strips of paper and thus obtained the width of each growth ring to the nearest hundredth of an inch.

Figure 1 is the smoothed growth curves for each age class of white pine, 1675 to 1925.

The author points out that in every age class from the 280-year-old stand to the youthful 75-year-old stand, which normally should be experiencing its most vigorous increment, there is a rapid decrease in growth during the last 40 years. This is so distinct as to preclude any possibility of chance being the cause. Suppression could not have been responsible because the trees studied were ones which from their size must have been dominants, or in youth even superdominants; therefore, it is held that the only possible solution seems to lie in a deficiency of precipitation.

The 40 years since 1885 have obviously formed an exceedingly dry epoch.

The author further says:

The evidence bearing upon the score of years between 1825 and 1845 also appears muddled at first sight. The 280-year class shows an exceptionally rapid decline, while the 180-year class reaches the trough of its first 140 years of growth. The 140-year class shows a slow acceleration, but relatively this can be considered a decided drop, for normally the period between 40 and 60 years should show the most rapid growth rate. Only the 230-year class is inconsistent, for it practically maintains its growth peak. Nevertheless, the vote seems to be 3 to 1 that this was a dry period.

Between 1785 and 1825 the 280-year class exhibits a remarkable peak, almost incredible in a stand which was already 140 years old. This certainly indicates an abundance of precipitation in a striking manner, as does the next younger group, which after 50 years of poor growth at the age of 95 started a rapid acceleration which lasted for 35 years. Only the 180-year class causes scientific sorrow, for no 40-year old stand should slump, no matter how slightly, during wet years. But here again the majority should prevail, pending further investigation, and so this 40-year period should be called a wet one on the growth records. * * *

Going back from 140 to 180 years ago, only two age classes remain. Both of these indicate clearly a dry period. The older drops rapidly and then maintains the lowest level of its first 180 years. The younger, just when it should be making its best growth, also reaches the low point of its first 180 years.

In regard to the 40 years before this period, we have only the oldest age class to fall back upon. This reaches a peak, as one would expect for a stand of 60 to 100 years, and we can only surmise from its unusual height, exceeding all other points on any of the curves, that this was due to a wet phase of the cycle coming in conjunction with the most vigorous period of youth.

—*A. J. H.*

WEATHER IN THE AMERICAS AS AFFECTING TRADE

[Cable reviews to Commerce Reports, Nov. 7, 1927]

Argentina, October 29.—Rains throughout the country brought about a brighter commercial outlook during October, and * * *. The sowing of cottonseed is in